Possibly some of your readers may have considered this problem, and may be able to suggest some other method of packing the spheres. I do not remember to have met with any discussion of it. W. STEADMAN ALDIS.

University College, Auckland, New Zealand, February 4

Temperatures in Lake Huron.

SOME very interesting results observed by Commander Boulton, R.N., on the temperature of the waters of the Georgian Bay, the eastern basin of Lake Huron, have been placed by him in my hands. They appear to establish that the waters of the bottom of the bay are colder than the even deeper waters of the rest of the lake.

Lake Huron in its profound depths forms three great basins—the Georgian Bay, defined along its western outline by the bold cliffs of the Niagara limestones, and the central and southern basins, separated by the subaqueous corniferous escarpment which diagonally crosses the lake in a south-eastern direction from the outline of Lake Michigan. Whilst the southern basin has generally a sandy bottom, and is in many parts comparatively shallow, the central basin has a floor chiefly of clay, and includes the deepest portions of the lake.

The surface temperature necessarily varies with the seasons, and with the continuous or fitful nature of the weather for days preceding the observations. Thus on May 11, 1888, when the ice had but recently broken up, the surface water of the Georgian Bay near Owen Sound registered 34° F., whilst at 15½ fathoms the minimum was 34½° F.

Observations will during the coming summer be continued in this and other lakes, but in the meantime the records given hereunder may be taken as preliminary illustrations of the temperature of the waters of the bay. For comparison, some published observations taken in 1860 by the United States engineers in the central and southern basins are also given.

GEORGIAN BAY.

Lat. 45° 6′, *long.* 81° 7′.
July 27, 1888, 8.30 *a.m.*

	° F.
Surface	60¼
10 fathoms	45¾
20 ,, ...	41½
35 ,, ...	41
66 ,, (bottom)	39½

Lat. 45° 35′, *long.* 80° 49′.
August 20, 1886, 8 *a.m.*

	° F.
Surface	59¾
31 fathoms (bottom)	39½

Lat. 45°, *long.* 80° 52′.
August 20, 1886, 12.38 *p.m.*

	° F.
Surface	65
42 fathoms (bottom)	37¾

CENTRAL BASIN.

Lat. 45° 18′, *long.* 82° 23′.
July 30, 8 *a.m.*

	° F.
Surface	52
65 fathoms (bottom)	42

SOUTHERN BASIN.

Lat. 44° 33′, *long.* 82° 54′.
August 5, 10 *a.m.*

	° F.
Surface	58
38 fathoms (bottom)	52

Lat. 43° 46′, *long.* 82° 1′.
June 20, 9 *a.m.*

	° F.
Surface	55
45 fathoms (bottom)	52

On August 20, 1886, the temperature of the surface rose from 59¾° F. at 8 a.m., to 62° at 9 a.m.; 63¾° at 11.34 a.m., and 65° at 12.38 p.m.

The suggestive explanation of the lower temperature of the Georgian Bay depths is that whilst the more southern and warmer waters of Lake Michigan in their course from the inlet to the outlet do not reach the bay, a considerable portion of the colder waters of Lake Superior find their way into it by the channel north of the Manitonlin Islands. Further, the subaqueous cliffs which block the western side of the bay preclude a free circulation between the deeper waters of the bay and the profound depths of the lake beyond. A. T. DRUMMOND.

Will Fluctuations in the Volume of the Sea account for Horizontal Marine Beds at High Levels?

IN the interesting article "On the Gradual Rise of the Land in Sweden" (NATURE, March 21, pp. 488-92), Nordenskiöld arrives at the conclusion that the small alterations of the relative level of sea and land which observation proves have taken place in Sweden, are due to movements of the land, not to fluctuations of the sea-level. On the other hand, he contends that the extensive horizontal stretches of marine strata found in many places on the earth's surface at heights measured by thousands of feet above the sea-level indicate fluctuations of level in the sea itself. This is certainly reversing the order of things as believed in by most geologists. It is also suggested that the fluctuations of sea-level are due to alternate increase and decrease of the volume of the sea, arising from gaseous and fluid additions from outer space or loss thereto, the alternate gains and losses balancing one another over long periods.

It is not my object in this communication to discuss the physical possibility of such alterations of the volume of the sea having taken place in this way, but to point out that, even if granted, such rising and falling of the sea-level fails to explain the geological phenomena for which it is invoked. Formations horizontal in one place are disturbed in another. They cannot be divided into two hard and fast stratigraphically dissimilar kinds of marine deposits, the *horizontal* and the *folded*, as is attempted by Nordenskiöld. Even the example quoted by him of the Tertiary strata of Spitzbergen shows this, as it is stated, "Near the west coast they are much disturbed, but further inland they form horizontal strata of sand and clay, &c."

The plains of Russia are, as was shown long ago by Murchison, largely occupied by nearly horizontal strata of undisturbed Silurians, while in the Ural Mountains the same formation is thrown up on end. I venture to pronounce this continuity of horizontal with disturbed deposits an almost universal phenomenon, for where plateaus are capped by horizontal strata, as often happens, these cappings are only the remnants left by denudation.

It is a well-known geological fact that as strata recede from a mountain range they become less and less disturbed and more horizontal. Again, there are no horizontal strata of any extent or thickness that are not riven with faults showing that they have been subjected to upthrow or downthrow as the case may be, and these have to be accounted for as well as the level at which the strata occur. Except in the very newest deposits, strata bear very little relation to the levels at which they are now found. Because strata are often horizontal at high levels it is no indication that they have not been upheaved. The Colorado plateaus may be cited as an instance, and such instances may be multiplied to any extent.

There is, however, another difficulty appertaining to the explanation offered by Nordenskiöld. It is this, the general rise of the sea-level over the whole globe to the extent even of 1000 feet would obliterate an enormous area of land. Where, then, would the sediment come from to form the beds appealed to in proof of the rise of the sea-level? Formations are not arranged concentrically at varying levels or, in other words, stratigraphically contoured, as would be the case were they due to this cause. But there is a final and still greater difficulty to be met. Denudation is year by year reducing the height of the land, and if no compensatory elevation excepting over disturbed areas took place, continents instead of growing as they are supposed by some to do, would long ago have been obliterated, and the earth planed down to a uniform level, so that when periods of "high water" recurred all terrestrial life would be destroyed. This contingency no doubt to some minds will be plain demonstration of the truth of Nordenskiöld's theory.

T. MELLARD READE.

Park Corner, Blundellsands, near Liverpool, March 25.

The Meteorological Conditions of the Aruwhimi Forest Tract.

I CANNOT but think that the true explanation of the rank exuberance of the Aruwhimi forests, so graphically described by Mr. Stanley, or rather of the humid climate that fosters them, is different from that suggested either by the great traveller himself or the writer of the notice in last week's NATURE. The source of the winds that feed the rainfall of this region seems to me a question of secondary importance, but since in equatorial regions, as a rule, easterly winds predominate, I am inclined to think, with the writer of the article, that this source is most probably the Indian Ocean.

If, however, this be so, since in the interval between the coast and the Aruwhimi basin they have to pass over some of the highest mountains in the continent, and reach the latter on a descending slope, they would be comparatively dry winds, more or less analogous to the Alpine *föhn*, were there not other conditions present which more than counterbalance the desiccating influence of the eastern mountains. The first and most

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important of these conditions I take to be the equatorial position of the Aruwhimi basin; the second that it is situated in the heart of the continent. Both of these, but more particularly the former, determine it as the seat of ascending air-currents, and therefore of their dynamic cooling on a gigantic scale, and it is to this dynamic cooling that the high rainfall of the region is to be ascribed.

Very probably a considerable portion of the precipitated moisture is locally re-evaporated, so that, as suggested long ago by Sir John Herschel in the case of the Brazilian forest rainfall, the same water is precipitated again and again. There are not, I believe, in the lower atmosphere, any steady winds blowing outwards to carry away the evaporation of the damp forest tract, and the main loss of water to be supplied by easterly or other winds is that carried off by the river drainage, probably less than half of the rainfall. The air which has ascended to the higher regions of the atmosphere as a part of the main circulation of the globe, parts with nearly the whole of its vapour in the act of ascending.

We have a case in some respects analogous to that of the Upper Aruwhimi in the very damp and equally forest-clad province of Upper Assam. This too is characterized by a very calm atmosphere, being girt with lofty mountains on the north and east, and also shut off on the south and south-west from the Bay of Bengal by hills of considerable elevation. Such gentle winds as blow in the valley are chiefly from the east or down valley. Yet the rainfall is over 100 inches in the year, and the whole tract is one of marsh and dense forest. It is indeed not situated under the equator, and herein it is less favourably conditioned as a region of excessive rainfall than the basin of the Aruwhimi.

As the result of a long study of the rainfall of India, and perhaps no country affords greater advantages for the purpose, I have become convinced that dynamic cooling, if not the sole cause of rain, is at all events the only cause of any importance, and that all the other causes so frequently appealed to in popular literature on the subject, such as the intermingling of warm and cold air, contact with cold mountain slopes, &c., are either inoperative or relatively insignificant.

Folkestone, April 11. HENRY F. BLANFORD.

"Les Tremblements de Terre."

M. FOUQUÉ's letter (NATURE, March 28, p. 510) does not meet the main points of my criticism of his book. He thinks that a pendulum swinging in synchronism with the ground's motion is the right thing to use as an absolute seismometer. M. Poincaré's mathematical note, to which he refers as supporting his view, does not support it, but shows why such a pendulum is unsuitable. It is necessary to emphasize this, for it relates to a fundamental matter in the dynamics of earthquake measurement—a matter on which the work done of late years in Japan seems to me to be so intimately based that a misunderstanding about it must be fatal to a proper appreciation of that work. And, in point of fact, I did not find that M. Fouqué gave an appreciative account of what any of the Japanese observers had done. As to his mention of Prof. Ewing's seismograph, in particular, I criticized it not so much because it was meagre as because it was incorrect,—so incorrect as to justify the inference that the author was not acquainted with that instrument.

THE REVIEWER.

Hertz's Equations.

MR. WATSON's criticism, that Hertz's equations are only true for places at some distance from the oscillator, is no doubt perfectly valid. [There is, by the way, an insignificant and obvious misprint of λ for ρ about the middle of his letter.] But this was entirely recognized by Hertz himself; he treated the oscillator as infinitesimal, knowing that it was nothing of the kind when you got near it, and refrained from drawing his diagram-curves into its neighbourhood, for this very reason.

The fact is surely that, to work out completely the case of electric oscillators in a compound body formed of a couple of spheres joined by a cylinder, would tax the resources of a strong mathematician; and it is impossible that the vibration can be, in any sense, a pure one; all manner of sub-vibrations must be superposed upon the main.

From the physical point of view, some general notion of what was happening at a distance of a wave-length or more from the oscillator was desirable, and this Hertz satisfactorily obtained.

But, to work out what is happening in the immediate neighbourhood of a dumb-bell oscillator must be left, I imagine, to the time when some pure mathematician may devote his attention to this particular shape of conductor, if the case appears to him of sufficient interest. At present I see no special reason why it should be so regarded, but of that Mr. Watson is a better judge. I hope he may see fit to attack the problem.

Grasmere, April 13. OLIVER J. LODGE.

THE COMPRESSIBILITY OF HYDROGEN.

AS stated in the obituary notice that appeared in NATURE (vol. xxxviii. p. 598) at the time of the melancholy accident which caused his death, Wroblewski was engaged in an investigation of the behaviour of hydrogen on compression. The results of this investigation, as far as it had then advanced, have now been made public (*Monatsh. für Chem.*, 1888, p. 1067 *et seq.*). They are of a most important and interesting nature, and form a fitting memorial of the patience and skill of the observer, who most unhappily was not spared to bring this, the last and most complete of a long series of similar investigations, to a close.

Hydrogen has long occupied an exceptional and isolated position among gases. This is due to the fact that, as Regnault first pointed out, hydrogen forms the sole exception to the law that the product of the pressure into the volume, pv, of any gas decreases with increasing pressure, the exact converse being true in the case of hydrogen, this product showing a regular increase. It is true that, as since shown by Amagat and others, this behaviour of hydrogen becomes general for all gases when the pressure is increased beyond a certain limit, but before reaching this limit the product pv invariably decreases until a minimum is reached for all gases with the exception of hydrogen. For hydrogen neither the decrease nor the minimum have yet been observed, the gas as hitherto examined showing an invariable increase of pv with increasing pressure. The natural inference was, however, that the exception was only apparent, and that the minimum above noted would be found to occur also with hydrogen if the gas were examined at lower pressures than those hitherto investigated—that is to say, at pressures below one atmosphere. But a difficulty in the way of this hypothesis arises from the fact that the critical pressures of all gases are found to be *below* the pressure at which the minimum value for the product of pressure into volume occurs, and therefore on the above reasoning the critical pressure of hydrogen would have to be phenomenally low and considerably beneath one atmosphere.

To gain a further insight into the relation of volume to pressure in the case of hydrogen, Wroblewski decided to investigate this relation through a wide range of temperature. For this purpose he selected as temperatures sufficiently apart, the boiling-point of water, 100° C., the melting-point of ice, 0° C., the boiling-point of liquid ethylene, $-103^\circ\!\cdot\!5$ C., and the boiling-point of liquid oxygen, -183° C. The pressures employed varied from one to seventy atmospheres.

The method of experimenting was exceedingly simple. The gas at a known pressure was forced into a bulb of known capacity having a capillary neck, and kept at one of the above four temperatures. A sufficient length of time was allowed for the gas to attain the fixed temperature; it was then transferred to a eudiometer, and its volume measured. It is needless to add that every precaution was taken both in purifying the gas and in applying the necessary corrections.

The results with the three first of the above temperatures agree with the behaviour of hydrogen already observed, the product of volume into pressure constantly increasing with the pressure. It was found that for the range of pressures under investigation (one to seventy

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